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ROYAL ASTRONOMICAL SOCIETY.

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Total Eclipse of the Moon, 1870, *July* 12. By the Astronomer Royal.

I made no observations of the eclipse, except in regard to the intensity of illumination of the Moon's disk at the middle of the eclipse. I remarked as follows :—

1. The difference in the intensity of illumination on the northern portion of the disk (least illuminated) and the southern portion (most illuminated) was much greater than I expected to see in an eclipse in which the Moon passed so near to the centre of the shadow.

2. With my eyes unarmed, being extremely short-sighted (the distance for distinct vision less than 5 inches), and every luminous object being seen as a broad blurr, I can compare the quantity of light from sources of very different character. Thus I found that the quantity of light received from the Moon was less than that from *Saturn* and greater than that from α *Aquilæ*, including some from β and γ *Aquilæ*.

3. The light of the Moon increased considerably in ten minutes after the time of central eclipse.

4. I infer from Nos. 1 and 3 that the only part in which the shadow is nearly total is the centre of the shadow, and that a large amount of light falls within the geometrical limits of the true shadow.

July 18, 1870.

On the Lunar Eclipse, 12th July, 1870.

By C. H. Weston, Esq.

I had hoped, with an Observatory commanding from its elevated position an angle of more than 90° declination, and an uninterrupted eastern horizon belonging to a circle of 120 miles in circumference, to have well witnessed the "First contact with the Shadow" on the Moon's Eastern Limb, north of her equator, and near the Ringgebirg *Olbers*. A fixed bank of clouds, however, prevented any observation until the Moon had risen about 8°, when the Earth's shadow was found to have already covered *Hevel*, *Cavalerius*, and parts adjacent to *Reiner*, and had reached the resplendent cliffs of *Aristarchus*, and the almost equally brilliant highlands between *Aristarchus* and *Herodotus*.*

At this time the whole of the western edge of the shadow was dark, while the east periphery of the Moon was brightly visible. In 10 minutes the shadow north of the equator had reached the base of the great *Copernicus* and districts not far east of *Tycho* (including part of his radial system) on the south; then passing *Eratosthenes*, it touched the southern extremity of the mountain-range of *Apenninus* three minutes later.

After an interval of six minutes the extensive surface, including west of *Sinus Iridum* on the north, and those of the more southerly *Archimedes*, together with the greatest part of *Mare Nubium* and *Tycho* on the south, was observed in shadow.†

At this time the east periphery was reddish, and the western edge of the shadow orange, resulting from the combination of the red tinge and the yellow lunar light beneath. About six minutes later the shadow had reached the districts of the *South Pole*, and near the eastern part of *Mare Serenitatis*. Before the Moon had been so far obscured the *Sinus Iridum* was visible, but after reaching the last described limits all the shaded surface showed its details—the absence of light allowing perhaps a greater dilatation of the pupil of the eye. Now *all* the eclipsed parts were reddish, and the western margin of the shadow purplish.

After 15 minutes the shadow reached as far as the western edge of *Mare Serenitatis* and the eastern portion of *Mare Tranquillitatis*, and on the north, near *Hercules*, and not far from *Endymion*. In two minutes the eastern margin of *Mare Fecunditatis* was in-

* The telescopes used were Newtonians, 9 and 14·25 inches and 9 and 16 feet respectively — the former on the back of the latter — on the same equatoreal mounting, and thus most readily available for comparative observations.

† During the eclipse, and some days before and after full moon, the region of *Archimedes* was well seen. Besides this Ringgebirg there were two others visible, nearly as large in circumference, but apparently less in elevation, than *Archimedes*, more resembling incipient rings common on other parts of the lunar surface. These were clearly the "incipient or low protrusions, *circular and rugged*," mentioned in my paper of December last, and but indistinctly seen in the earlier phases of the Moon.

volved, and two minutes later the eastern part of *Mare Crisium* and the Ringgebirg *Geminus* further north. At this time the bright western crescentic limb of the Moon, contrasted with the eclipsed portions, afforded a fine example of the enlarging effects of irradiation. The disturbed district between *Mare Crisium*, *Mare Tranquillitatis*, and *Mare Fecunditatis*, were finely brought out just before entering the penumbra, and *Proclus* (nächst *Aristarch* das hellste Ringgebirg, B. & M.) was strikingly prominent.

After an interval of five minutes the shadow extended to the west of *Mare Fecunditatis*, and nearly west of *Mare Crisium*, and about three minutes later the phase was total.*

Now light appeared gradually to extend itself eastward over the lunar surface, and in about eight minutes after the total eclipse the converging ray-system of *Tycho* on the south, and all the country east and west of *Sinus Iridum* on the north, came out to view, while the region about the first impact was still darkish. In about 40 minutes the light was more uniformly diffused over the Moon, and after quarter of an hour the illumination was pretty equal on the east, and west, and south, while the north was darkest. After 10 minutes the east side was the brightest. Small star near the S.W. limb was well seen.

After six minutes the south-eastern periphery was brightest. *Tycho's* circular structure loomed out. The Moon gradually became darker after the mid-phase, and then the western parts also gradually less visible. *Aristarchus* was a very brilliant object.

Soon the reddish tint on the north-west appeared, which in about four minutes extended over the surface towards the south-east. Then came symptoms of approaching dawn, and two minutes later the north-east above *Grimaldi* began to brighten. Then the concave band of solar light appeared on the lunar disk, passed over *Grimaldi* and the disturbed regions east of, but distant from, *Gassendi*, and in four minutes illuminated the south-east of *Mare Humorum*. Clouds now began to cross the Moon.

Later (six minutes) the light dawned upon *Cichus* (eine grosse Vertiefung, B. & M.), and further south to the south-east of *Tycho*. More clouds. Later obtained a glimpse, and found the light had reached *Tycho*. Another 20 minutes, and the west of the north end of the range of *Apenninus* was illumined. Nothing afterwards was visible.

During the progress of the eclipse the advancing convex shadow of the Earth was observed to produce on the Moon's spherical surface a series of arcs, not *concentric* but *convergent* towards the north periphery; and it should be noted that the returning light impinged first at a point not identical with the shadow's first contact (which must have been near *Olbers*), but at a point a little

* The region between *Schubert* and the western periphery of the Moon (the last portion involved in shadow) was apparently more extensive than in Mean Moon, and doubtless arose from her libration, the maximum of which would occur two days later.

north-east of *Grimaldi*, several degrees further south than *Olbers* —visible effects of the different relative positions and motions of the Earth and Moon during the period of the eclipse.

Ensleigh Observatory,
Lansdowne, Bath, July 1870.

On Mr. Browning's Spectroscope. By M. C. de Littrow.

(*Note addressed to the Foreign Secretary.*)

I take the liberty to let you know that the essential idea in Mr. Browning's arrangement of the spectroscope (*Monthly Notices*, vol. xxx. p. 198) has already been executed and published by my deceased son Otto, in the year 1862, as you will find in the annexed treatise (*Proceedings of the Imperial Academy of Sciences of Vienna*, vol. xlvii.). The only difference between the two instruments consists in the sparing of the second telescope, which my son has effectuated by placing a mirror behind the last prism, so that the same telescope serves also as a collimator. Besides this advantage, he obtained thus the double effect of the employed prisms. He enclosed the whole apparatus in its box, so that he needed not darken the room in which he experimented.

I beg the favour of bringing this short note to the knowledge of the readers of your *Monthly Notices*.

Vienna, July 18, 1870.

Note on the Automatic Spectroscope. By John Browning.

When constructing the automatic spectroscope which I have recently had the honour of describing to the Society, I saw the desirability of giving a motion to the telescope without the use of a cam. Having spoken to Mr. Proctor on this subject, he told me that he had been convinced, while the new spectroscope was under discussion at the last meeting, that my principle really involved the complete solution of the problem of securing minimum deviation for all orders of light-waves. It was only necessary for the purpose that the slot movement should be extended to the first prism, and to the viewing telescope. It is obvious that in this way the necessary amount of motion will be communicated to the telescope, and also to the first prism, which in the former arrangement was a fixture. I am making an instrument containing this improvement, and hope to have the honour of exhibiting it at the next meeting of the Society in November.

111 *Minories, July* 2, 1870.

Note on Mr. Browning's Automatic Spectroscope.
By Richard A. Proctor, B.A.

It occurred to me, while Mr. Browning's spectroscope was under discussion at the last meeting of the Society (and more particularly when Prof. Pritchard raised the question whether all rays pass through the battery with minimum deviation for each prism), that by a slight modification Mr. Browning's ingenious device was capable of more completely fulfilling the conditions of the problem suggested by Prof. Pritchard. I am not sure that practically the spectrum will be viewed under much more favourable conditions by the plan I am about to propose; but fortunately the plan has (Mr. Browning tells me) other advantages. I need hardly say that I only present it as a modification of his most ingenious plan, and claim no separate merit for the new arrangement, even though the optical performance of the battery of prisms should be appreciably improved by it.

It is necessary to premise that although it is possible, as I shall show, to secure minimum deviation for rays of all degrees of refrangibility, yet we can obtain only an approximation to that condition which minimum deviation is intended to secure. Even in the case of a single prism, the primary and secondary foci of the emergent pencil corresponding to rays of given refrangibility are not absolutely coincident when such rays pass through the prism with minimum deviation. If φ and ψ' represent the angles of incidence and emergence, φ' and ψ the angles of first and second refraction, we have for the distance between the primary and secondary foci of the emergent pencil the expression

$$\left[\frac{\cos^2\varphi'\cos^2\psi}{\cos^2\varphi\cos^2\psi'}u + \frac{t\cos^2\psi}{\mu\cos^2\psi'}\right] - \left[u + \frac{t}{\mu}\right]$$

where u represents the distance of the origin of the source of light from the point of incidence, t the length of the path within the prism, and μ the refractive index of the prism for the rays considered. Hence, that the primary and secondary foci of the emergent pencil may coincide, we must have

$$(\cos^2\varphi'\cos^2\psi - \cos^2\varphi\cos^2\psi')\,\mu u = t\cos^2\varphi\,(\cos^2\psi' - \cos^2\psi).$$

Only when t vanishes does this reduce to the condition

$$\cos^2\varphi'\cos^2\psi = \cos^2\varphi\cos^2\psi'$$

or

$$\varphi = \psi',$$

which is the condition of minimum deviation.

Now t is ordinarily of appreciable length for each prism, in the case of a battery of prisms; and therefore the resulting distance between the primary and secondary foci is also appreciable, though small, even in the case of the first prism of the battery,

and will increase with each successive prism. The expression for this distance reduces, in the case of minimum deviation, to

$$\frac{t}{\mu}(1-\mu^2)\tan^2\psi',$$

or, if the refracting angle of the prism be $2i$, to

$$\frac{t}{\mu}(1-\mu^2)\tan^2 i.$$

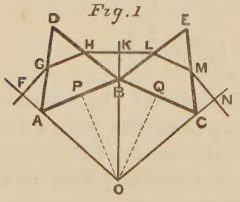
The circle of least confusion will therefore have a radius represented by

$$\frac{t\lambda\,(1-\mu^2)\tan^2 i}{\mu u\,[\sqrt{1-\mu^2\sin^2 i}+1]+t\,[(1-\mu^2\sin^2 i)^{\frac{3}{2}}+1]}$$

where λ is the breadth of the pencil at the place of emergence. It is obvious, however, that this expression will be minute under the ordinary conditions of spectroscopic observation.

Setting the condition of minimum deviation, instead of the true optical condition, as that which we are to aim at, it is obvious that in the case of a battery of prisms, all of equal refracting angle, what we require is that, first, the bases of all the prisms shall always circumscribe a circle; and, secondly, that the angle of incidence on the first surface of the first prism shall be equal to the angle of emergence from the second surface of the last prism. If these conditions are fulfilled, the condition for minimum deviation must necessarily be fulfilled; if they are not fulfilled, there cannot be minimum deviation.

Fig. 1



Suppose, for instance, that A D B and B E C (fig. 1) are two prisms of equal refracting angles, D and E, and connected, as in Browning's spectroscope, at the angle B; and let F G H L M N be the path of a ray refracted with minimum deviation through each. Then

$$\cos \mathrm{B\,H\,K} = \mu \sin\frac{\mathrm{D}}{2} = \mu \sin\frac{\mathrm{E}}{2} = \cos \mathrm{B\,L\,H};$$

that is, B H L is an isosceles triangle, and O B K, the bisector of the angle D B E, is perpendicular to H L.

Now P O and Q O, the rectangular bisectors of A B and B C, meet on B O, as at O; and O A F makes with G A the angle G A F, equal to the angle H B K. But the angle A G F is equal to the angle B H K. Hence the angle A F G is equal to the angle B K H, or is a right angle.

In like manner O C N is at right angles to M N.

If another like prism be added at C, the same reasoning will apply; and so on, whatever be the number of prisms.

It follows that the lines from a single point O to the attached angles A, B, C, &c. of any such series of prisms are perpendicular to the course of the ray between the prisms. But any two perpendiculars O P and O Q from this point O on successive base-lines A B and B C are obviously equal; hence all such perpendiculars are equal; and therefore A B, B C, &c. are all tangents to a circle about O as centre. Further

$$\angle \text{FGA} = \angle \text{BHK} = \angle \text{BLH};$$

that is, the angle of incidence on any prism of the series is equal to the angle of incidence on the next. Therefore the angle of incidence on the first prism is equal to the angle of incidence on the last, and therefore to the angle of emergence from the last.

These are the conditions, then, which must be fulfilled in all positions of a self-adjusting battery, if rays of all degrees of refrangibility are to pass with minimum deviation through each prism, viz.

1st. *All the bases must be tangents of a circle.*

2dly. *The pencil must emerge from the last prism at the same angle at which it fell on the first prism.*

The first condition Mr. Browning's plan obviously fulfils, for the perpendiculars bisecting the equal bases all pass through one point.

The second condition the plan does not, as first devised, accurately fulfil, because the first prism is fixed, and the incident white light falls on it from the slit at a constant angle; so that there can be minimum deviation only for rays of a certain degree of refrangibility.*

The modification I suggest consists merely in fixing an angle (the angle corresponding to A in fig. 1) of the first prism, and causing this prism to move so as that the line corresponding to O A F shall be constantly perpendicular to the axis of the incident pencil FG. Since the axis of the incident pencil is fixed in position, this involves only the addition of a fixed slot in the position O A F.

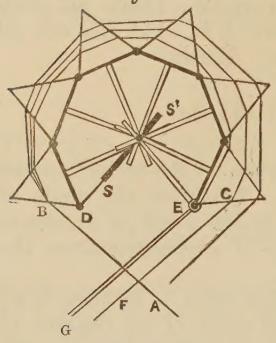
It further occurred to me that since by this arrangement the axis of the emergent pencil from the last prism is situated (as M N in fig. 1) in a direction at right angles to the line from O to the angle corresponding to C, all that is necessary to keep the viewing telescope in its true position, whatever part of the spectrum is studied, is that a slot coincident in direction with this

* Thus, if the constant angle of incidence be ϕ, and the refracting angle of each prism $2i$, minimum deviation is secured only for rays whose refractive index is $\frac{\sin \phi}{\sin i}$.

last-named line should be rigidly attached to an arm bearing the telescope, and should be also at right angles to the optical axis of the telescope.

In fig. 2 the actual position of the prisms is indicated. A B is the axis of the incident pencil at right angles to the fixed slot S S′ (whose direction passes through the angle D of the first prism). The double lines indicate the position of the movable slots, the last of which, O E, is rigidly attached to the viewing telescope, and at right angles to its axis, which corresponds in position with C F, the axis of the emergent pencil.

Fig 2



To obtain the greatest possible dispersion, without losing any part of the spectrum, the refracting angles of the prisms should of course be such that E G will just touch the angle D when the extreme limit of the visible spectrum at its most refrangible end is in the field.

I may add that if A B (fig. 3) be the base of any one of the prisms of the series, it will be evident from the figure that the prism A C B may be replaced by a prism E D F (A E being equal to F B, and E D, F D parallel respectively to A C, B C) without affecting the subsequent path of the *axis* of any pencil; and that for certain purposes the shortening of the path within the prism by such a change would be advantageous. I need not enter into the consideration of the other ways in which a similar shortening can be effected; but I may note that there seems nothing to prevent the equal prisms used in Mr. Browning's present arrangement from being replaced by a series of increasing prisms, represented in section by the shaded triangles in fig. 4 (the last prism of the battery remaining unchanged). If this arrangement is practically feasible, the definition would, I imagine, be appreciably improved. As the figure shows, the course of the axis of a pencil would not be so altered as to affect the path between the prisms, or the equality of the angles of incidence and emergence.

Fig. 3

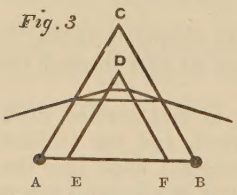
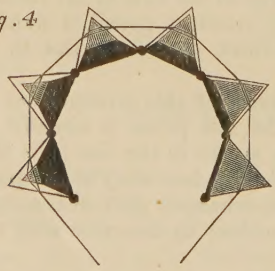


Fig. 4



Since the above was written, I have devised a plan by which

minimum deviation can be secured automatically for a battery of nine, ten, or eleven equilateral prisms. The light from the sixth prism of a series like the above (figs. 2 and 4) is received on a totally reflecting prism, from which it passes to a second battery in a reversed position. By a very simple contrivance the totally reflecting prism adjusts itself, so as always to hold a symmetrical position between the two batteries, the faces including the right-angle of the prism being always perpendicular to the light passing out of one battery and into the other. But as a long connexion, such as this arrangement involves, necessarily works imperfectly (however true theoretically), I at once double the dispersive power and halve the effective length of the connexion, by causing the light to be reflected so as to re-traverse the whole battery, and to be viewed by a fixed telescope whose optical axis is in part coincident with that of the collimator. The motion of adjustment thus has to be communicated to the totally reflecting prism, and the effective length of connexion is the same as for a single battery. I hope to be able to describe this plan in full at the next meeting of this Society. It need hardly be said that the arrangement is only useful where there is abundance of light. One secures by means of it a dispersion corresponding to that given by 22 or 23 equilateral prisms (according to the way in which the light is sent back through the battery); but the power can be reduced prism by prism to any extent which may be requisite.

The Satellites of Uranus. By Richard A. Proctor, B.A.

It has been with extreme regret that I have seen a passage in my *Other Worlds* interpreted by a Vice-President of this Society as a setting up of my mere opinion against the telescopic observations of our President. I hasten to correct this misinterpretation. In expressing the opinion that Sir William Herschel was not deceived as respects the four satellites of *Uranus* which remain unidentified, I was but doing what Admiral Smyth, Professor Nichol, and Professor Grant, had already done (simply on the score of Sir William Herschel's wonderful accuracy as an observer), I am convinced that Mr. Lassell (whose name, by the bye, I had not mentioned in the matter) would be the last to condemn this confidence in the work of an astronomer whose achievements he has so worthily emulated.

I wrote, it is true, with full knowledge that Mr. Lassell has not detected other satellites than *Umbriel*, *Ariel*, *Oberon*, and *Titania;* and with full knowledge also of the qualities of his four-feet mirror. But Sir William Herschel's observations indicate a variability in the lustre of the satellites quite sufficient to account for Mr. Lassell's want of success (so far): and though the light-gathering power of Herschel's four-feet mirror was doubtless inferior to that of Mr. Lassell's, yet it must be remem-

bered that Herschel could not see the satellites when his telescope was used (like Mr. Lassell's) as a Newtonian.*

As Professor Pritchard, at a recent meeting of this Society, spoke of himself as one of scarce three (he thought) present who had read all Sir William Herschel's papers, he will not need to be reminded that Herschel (to use Professor Grant's words) "asserted his firm belief in the existence" of these other satellites. I confess that I should require *very* strong negative evidence to force on me the conviction that Herschel was mistaken on any occasion when he expressed confidence about an observation. Writing with the remembrance of Sir William Herschel's confidence strong upon me, I said in *Other Worlds* that "one cannot read the account of Herschel's method of procedure without feeling that no amount of mere negative evidence can be opposed effectively to the positive information he has left respecting those four orbs." Is this, I would ask, to be considered as an inexcusable attack (as Professor Pritchard has implied) on our esteemed President, who has, indeed, taught us to believe in unseen satellites, *since his name will for all time be associated with the discovery of an eighth Saturnian satellite long after astronomers had concluded that but seven exist?*

Further Note on the Change of the Colour in Jupiter.

By John Browning.

Since I sent in my Note in reply to the Astronomer Royal's remarks, which appeared in his Report to the Board of Visitors, I have been favoured with a copy of a paper by Prof. A. M. Mayer, of Lehigh University, Pennsylvania, U. S. As this paper appears to me to fully corroborate the substance of the three papers I have had the honour of giving to the Society, I now venture to present some extracts from it :—

"Every astronomer who, during this fall and winter, has made careful observations of *Jupiter*, must have remarked the unusual colours of his disk and belts, and the remarkable forms and mutations which the latter have frequently presented."

* * * * *

"The colour of the planet next demands our attention, and surely no one familiar with the usual tints of the belts and of the general surface can fail to remark the unusual colours which this drawing exhibits.

"*Jupiter's* disk generally is of a light yellow colour crossed by

* Professor Grant thinks it likely that Sir William Herschel refers to his 20-feet mirror, not to the 40-feet one (which was difficult to use) in his account of the discovery of these satellites. But although Sir W. Herschel does not mention specially the 40-feet telescope, it is evident from his remark about the use of the "front-view" (coupled with his original description of that method of using large reflectors) that it was his great mirror he employed. Otherwise, indeed, the whole matter would be infinitely perplexing.

belts of a brownish-grey tint; sometimes, though rarely, approaching a rose colour. Often the brown is entirely wanting, and the belts are merely dull streaks on a light yellow ground.

"We can best convey an accurate conception of the colouring, seen on the 5th of January, by stating the manner in which we tinted the drawing, which, when compared repeatedly at the telescope by myself and others, was found correctly to give the colours and their relative intensities.

"The whole disk was first washed over with a slight tint of yellow. The colour of the region between the equatorial belts was correctly reproduced by pure yellow mixed with crimson-lake, while the two dark belts north, and the one south of the equator were obtained by the same combination of colours, with more crimson-lake added, so as to bring the tint to approach a coppery hue. The polar shadings were correctly given by pure yellow with a slight dash of crimson-lake, which tinting was subsequently overlaid with a very thin wash of light lead colour."

Reading over the last paragraph, one might imagine it to be a description of the last coloured drawing which I have made of *Jupiter*. A copy of this drawing forms the frontispiece to Mr. Proctor's new work, *Other Worlds than Ours*. On referring to this coloured plate, the resemblance may be almost as clearly traced as by examination of the drawing. Professor Mayer used a *refractor* of six inches aperture, made by Alvan Clark.

Clapham, June 30th, 1870.

Further Remarks on the Corona. By Richard A. Proctor, B.A.

At the last meeting of this Society, Dr. Gould, of America, indicated his belief that the trapezoidal corona seen by himself and other observers during the progress of the American eclipse was in fact but the chromosphere seen under unusually favourable circumstances. He added, that the light outside that four-cornered corona appeared to shift in position, and hence he concluded that it was terrestrial.

It seems to me that if this view be admitted, the difficulty pointed out by Mr. Lockyer in the case of the corona considered generally, exists in scarcely diminished extent in the case of this trapezoidal appendage also. Estimated by most of the observers as extending fully 12′ from the disc of the eclipsed Sun, its real depth would be far more than 320,000* miles, and the pressure even at the summits of the highest prominences would be enormous.

We gain nothing, then, by Dr. Gould's supposition; though of course that does not prove it to be erroneous. But Dr.

* Even in Mr. Whipple's photograph it has an extent of fully 6′, which would correspond to more than 160,000 miles, or 80,000 miles above the highest prominences yet seen.

Curtis (whose successful photographs appear in Commodore Sands's reports of the total eclipse) remarks that he has read Dr. Gould's statements respecting the eclipse with considerable surprise. After referring to the photographic evidence, he adds, "Dr. Gould adduces as an additional argument in favour of his assumption the observation that the long coronal beams appeared to him to be 'variable,' while the 'aureole' photographed was evidently 'constant' during the time of totality. This argument, however, loses some of its force, when it is remembered that to other observers the corona appeared to the eye absolutely unchangeable, both in form and position, during the whole period of the total obscuration." He goes on to indicate the probability that Dr. Gould has mistaken a photographic effect for a real phenomenon, in this case, precisely as when he interpreted the apparent encroachment of the bases of the prominences on the Moon (a dark-room phenomenon, as Curtis shows) to "specular reflection" at the Moon's surface.

I must confess, that after a very careful study of the whole series of American observations, Dr. Gould's view appears to me to be altogether disposed of by the concurrent testimony of so many and such skilful observers.

One striking, and as yet unnoticed, piece of evidence exists in General Myers' report of the appearance of the corona as seen from the summit of White Top Mountain, 5530 feet above the sea-level. Here the same quadrangular aspect was observed as at lower levels (and in Whipple's photograph), but the rays were much longer. "The silvery rays," he says, "were longest and most prominent at four points of the circumference—two upon the upper, and two upon the lower portion—apparently equidistant from each other, and at about the junctions of the quadrants designated as 'limbs,' giving the spectacle a quadrilateral shape." He remarks that these silvery rays were "straight and massive," and extended "to a distance of two or three diameters of the lunar disc." He adds, "There was no motion of the rays."

It seems impossible to mistake the significance of these observations.

In my paper in the March number of our *Notices*, I dealt specially with the theory that the corona is due to the illumination of the Earth's atmosphere by light not affected "by any action at the Moon." Many of the arguments, however, apply equally well on the supposition that there is such action. The striking fact that at the time of central eclipse the cone within our atmosphere bounded by lines from the observer's eye to the Moon's limb, contains no light, while the cylinder within our atmosphere bounded by lines from the Sun's limb to (and produced beyond) the Moon's, contains much light, affords, I take it, absolutely convincing evidence that this light is derived from an object far beyond the Moon. For if we suppose the solar rays to get by any process within the cylinder, they should

clearly traverse the cone also. For example, assuming that a solar ray passing by the Moon's edge is deflected (by whatever cause) so as to fall within that cylinder into which (from its very nature) undeflected rays cannot pass, the deflection, in order to account for observed appearances, must carry the illumination of our atmosphere up to the above-mentioned cone, and there suddenly the illumination must cease. But the cone has no existence in nature; it is but a mathematical conception: why then should these deflected rays respect it?*

Even La Hire's theory, which De Lisle is supposed to have overthrown, seems more easily supported than one which requires a moving shadow-cylinder of air to be illuminated, while a fixed cone (*not* a shadow cone) within it remains in darkness.

It seems much more natural to regard the blackness of the lunar disc, and the relative brightness of the corona, as due simply to the fact that the Moon is an opaque body very much nearer to us than the corona.

Let me renew my statement that it is the importance of the approaching eclipse which forces me to urge now views which I have long entertained. It appears to me that if, as I hold to be the case, the evidence respecting the corona is amply sufficient to prove it to be a solar appendage, then it would be a serious misfortune if any observers were to devote their time to establishing this fact. Instead of this, I should be glad to see every moment of the short duration of totality devoted both by general observers and spectroscopists to the inquiry *what sort of a solar appendage* the corona may be. On this inquiry depend issues of the utmost interest and importance to science; the other would be a waste of time: on one question we have abundant evidence; on the other (to quote the just words† of Professor Pritchard), "wise astronomers profess their profound ignorance."

* Mr. Lockyer tells me that M. Faye expressly suggests that there is some action at the Moon, and that, according to his and M. Faye's theory, it is thus the atmosphere gets illuminated. What the nature of the action may be I have not yet heard, nor can I conceive of any which would account, however roughly, for observed facts. Mr. Seabroke's paper on the Corona in the last number of our *Notices*, assumes no such action to take place; yet it is supposed that he is there defending Mr. Lockyer's theory. On the other hand, let me note in passing, Mr. Seabroke deals with an imaginary (I had almost said impossible) eclipse, and is therefore evidently not attacking my views respecting real eclipses. His arguments are, however, mathematically accurate. At the very moment and place of second and third contacts in an ordinary total eclipse, or during the occurrence of an exact total eclipse (which could last of course but an instant), the results he educes would doubtless take place, though it would be wholly impossible to observe them. But his formulæ are wholly inapplicable to the circumstances of any actual eclipse.

† Just *per se*, though somewhat too magisterially applied as a warning to myself. I also profess complete ignorance as to the nature and condition of the material forming the corona, but it is on account of that ignorance that I am so anxious to see the skilled observers of the approaching eclipse employing the opportunity in the most advantageous manner.

The use of the Steel-Balance in estimating the Distribution of Stars and Nebulæ. By Richard A. Proctor, B.A.

It is often difficult to estimate the extent of any irregular district of the heavens, and hence the numerical statistics of star-distribution require to be supplemented by a means enabling us to deal readily with spherical areas. I propose for this purpose (and have already begun to apply) the following method. Having laid down any convenient isographic projection, let the boundaries of any space whose area we require be traced on that projection. Then let the included space be carefully cut out and weighed against the remainder of the projection, with any convenient form of the steel-balance. The area of the space thus becomes known, and the relative richess of star-distribution can at once be determined.

Suppose, for example, a certain region r contains n stars of a given order (or nebulæ, as the case may be), while the whole heavens (R say) contain N such objects. Suppose by means of the steel-balance we find the following relation between the weights w and $(W - w)$ of the regions r and $(R - r)$:

$$\frac{w}{W-w} = \frac{1}{a}\text{; then } \frac{w}{W} = \frac{1}{a+1}$$

and the diversity of distribution in the region r is to the average density of the whole heavens as $\frac{n}{1} : \frac{N}{a+1}$, or as $(a+1)\ n : N$.

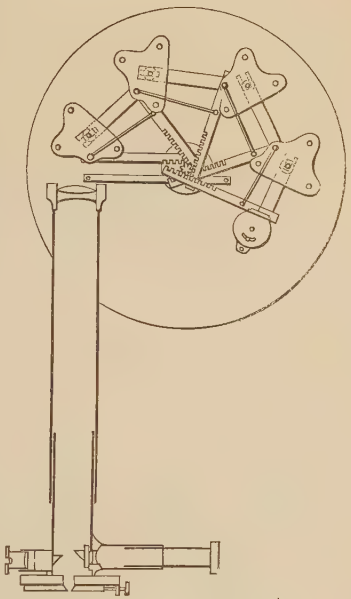
The construction of a polar isographic projection by the method described in my *Handbook of the Stars* (originally designed, I afterwards learned, by Sir John Herschel) is so simple a matter, and can be effected so accurately, that the plan described above affords a ready means of solving a number of problems connected with the areas of spherical surfaces. For example, I believe a more accurate estimate of the relation between the land and sea surfaces of our Earth could be obtained in this way than by the methods hitherto applied.

It is obviously quite as easy to compare any two celestial regions together by this method as to compare a given region with the whole heavens.

I hope at the next meeting of the Society to exhibit some results obtained by this method, which seem to indicate a surprising inequality in the distribution of stars in different portions of the heavens—I mean among those orders of magnitude within which, if the views at present accepted were true, a general equality was to have been expected according to the laws of probability.

Reply to M. de Littrow's remarks on the Automatic Spectroscope. By John Browning.

Having obtained a copy of the Number of the *Proceedings of the Imperial Academy of Sciences of Vienna*, vol. xlvii. which contains a description, with figures, of Herr von Littrow's Spectroscope, I am enabled to present the readers of the *Monthly Notices* with a copy of the principal diagram. On comparing this with the diagram of the instrument which I have contrived and described in *Monthly Notices*, vol. xxx. p. 198, I do not think any one will agree that, "the only difference between the two instruments consists in the sparing of the second telescope."



If I understand Herr Von Littrow's plan he has devised a symmetrical arrangement of the prisms, and communicated motion to each prism separately by means of a rack on a rod attached to the base of each prism, at right angles to the base, controlling this motion by means of cords. The adjustment of such an instrument must be necessarily tentative—each prism being brought right separately in the first instance; and it would be very difficult to so adjust it, that by a continuation of the rack-work motions, each prism should move with the requisite variable motion when the apparatus is put in action.

In the plan I have contrived, the prisms are so arranged that as the rod from the base of each can move freely over a common centre, and the whole of the prisms are linked together, a motion given to any part of the train of prisms must produce the desired effect, because the bases of the prisms remain always at a tangent to an inscribed circle, and, consequently, the angles between all the prisms remain the same.

I must disclaim having had any previous knowledge of Herr von Littrow's invention.

ERRATA

In Monthly Notices for June 10, 1870.

Vol. xxx. No. 8.

Page 203, line 20 from bottom, *for* Canis Venatica, *read* Canes Venatici.

— — No. of Observations of N.P.D. in 7-year Cat. (1860), *for* 8, *read* 18.

— 204, N.P.D. of Groombridge, 1830, for 1869,
for 51° 20′ 29″·84, *read* 51° 20′ 29″·98.
Annual Variation in N.P.D. for years 1864—1869,
for + 25″·72, *read* + 25″·75.
Proper Motion in N.P.D. for years 1864—1869,
for + 5″·71, *read* + 5″·74.

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